The Contributions of
Heinrich F.E. Lenz
to Electromagnetism

Wilbur Morris Stine

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BY
WILBUR MORRIS STINE

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PREFACE

BEYOND an impersonal knowledge of Lenz, derived, for the most part, from the law bearing his name, the general student of electricity has no acquaintance with the work, or character of Lenz. It may occasion surprise to more advanced students of physics to learn, that he was a broad, ingenious and tireless investigator both of pure and applied physics.

Though an alien to English readers, on account of his nationality, the name of Lenz is so preëminent — even among the illustrious scientists whom Russia has given to the world — that an extended knowledge of the historical value of his work is essential to the adequate comprehension of one of the most interesting chapters in physical science and its applications.

The essential purpose of the author has been to present that portion of the work of Lenz which was influential in establishing the science of electromagnetism; and to show somewhat of the personality of this investigator. This was considered preferable to a bare recital of his scientific labors. The accomplishment of this purpose necessitated a style and method, which is a departure from the conventional scientific writings.

The customary scientific method proceeds immediately to detail minutely the facts in the experiments and papers of the scientist, who engages its attention. At once it becomes occupied with detail and minutia, which it classifies with logical precision; but, as commonly, the burden falls upon the reader to derive from this recital the philosophical value and significance of the details.

The present work is a departure from this practice. It deals with a period in the history of science, which is without parallel in interest. By common consent to this period belong two preëminent names, Faraday and Henry. But to these a third, that of Lenz, must be added: and it has been the pleasant task of the author to consider in a philosophical spirit, both the period and the men from whose labors it has received prominence; and to assign to Lenz a place in the history of this period, scarcely less eminent than that accorded to Faraday or Henry.

The book has been written entirely from original sources; and is provided with footnotes and a bibliography sufficient for the most extended study of the scientific work of Lenz. It has been the outgrowth of a paper on its subject, read before the physical section of the Franklin Institute, April 24, 1901; and, later, printed in its *Journal*, April–May, 1903.

The original suggestion for this paper, and the present work was a monograph on the researches of Lenz, written by W. Lebedinsky, and published in the St. Petersburg *Electricity*, in 1895. This paper was translated for the author by a former student, Mr. Stone; and, to its suggestiveness, rather than details, this book is indebted.

The author is under obligations to the Librarian of Columbia University; and to Mr. Alfred Rigling, librarian of the Franklin Institute, for assistance in procuring copies of original papers; and to Mon. d'Oldenburg, secretary of the Imperial Academy of Sciences at St. Petersburg, for a list of biographical notices of the subject of this book.

WILBUR M. STINE

SWARTHMORE COLLEGE,
July, 1905

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H. F. E. LENZ . . . *Frontispiece*
From a portrait engraved in 1866.

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PART ONE

THE BEGINNINGS OF THE SCIENCE OF ELECTROMAGNETISM

THE CONTRIBUTIONS OF H. F. E. LENZ TO ELECTROMAGNETISM

CHAPTER I

ON THE HISTORY OF THE RISE OF THE SCIENCE OF ELECTROMAGNETISM

MODERN science may be said to have found no place, as yet, in its manifold activities for the purely historical research of its true beginnings. Characterized by the precision of its processes and the exactness of its knowledge, it has, perhaps, in all of its fundamental divisions developed from a past more or less remote, in which it was vaguely apprehended by philosophy, or dishonored in its practice by the charlatan and the pretender to occult powers.

This past, whose records are partial and obscured, affords a field for historical research — however difficult it may be to explore — which is as inviting and interesting as any subject of kindred investigation.

The few efforts of this character which have been made — to instance but the widely diverse work of Whewell [*] and of Benjamin,[†] — excite in the reader an intellectual interest, which is no less pleasing and sustained than that aroused by the political historian. Though the history of science may be directly devoid of sentiment; and may lack the human note in it, which lends the fascination to the narrative of the acts of a nation: yet, the account of the beginning and development of any one of the more important branches of science, possesses an intrinsic and peculiar charm for the

[*] *The History of the Inductive Sciences*, by William Whewell; 1837, 1840.

[†] *The Intellectual Rise in Electricity*, by Park Benjamin; New York, 1895.

reader, who can peruse it intelligently and is conversant with its technicalities.

The historical presentation of the subsequent experimental and analytical development of fundamental principles of a branch of science, is fragmentary, at best. The introductory chapter is often accorded to this in the treatise, whose purpose is the technical exposition of the principles themselves. Under such circumstances, the historical treatment is inevitably brief and fragmentary, and is developed merely to that extent which serves the author's present intentions.

These observations have been made in a somewhat general, or wide sense, so require more or less qualification. The well-informed reader will recall much admirable historical matter incorporated incidentally in various treatises, such as Preston's *Heat*, or *Light*,* where it forms an integral part of the discussion; also, Todhunter's *Elasticity and Strength of Materials;*[*] and, finally, Rosenberger's *Geschichte der Physik.*[†]

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* *The Theory of Heat*, by Thomas Preston; London, 1894: and *The Theory of Light*, second edition; London, 1895.

Doubtless such incomplete historical treatment has met the needs of the general reader and the technical student, who have their attention engrossed to such an extent by the applications of principles, that they have neither leisure nor desire for the details of the inception and development of a scientific principle. But these conditions must eventually give place to the demands of a broader scholarship, which will be more exacting and require a consecutive presentation of the development of a science from its real beginnings.

These remarks have a varying applicability to the different branches of science. As has been instanced, the history of the development of physics and elasticity, is already presented with some degree of thoroughness; the beginning and development of inorganic chemistry is, similarly, well presented in various technical and general works: but, in other branches of science, probably less attention has been given to the historical study of electromagnetism, than its like. The reasons for this are readily apparent, if the rapid development of this science, and the practical rather than academical aspect it has commanded, be considered. Books with varying degrees of pretension have not been lacking, which have borne the generic name, "Electricity and Magnetism". Definite histories of electricity have not been wanting, such as Albrecht's; * and, to supplement these, popular books of the character of Trow-

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[*] *A History of the Theory of Elasticity and of the Strength of Materials, from Galilei to the Present Time,* by Isaac Todhunter, edited and completed by Karl Pearson; Cambridge, England, 1886–1893.

[†] *Die Geschichte der Physik,* by Dr. Ferd. Rosenberger; Braunschweig, 1882–1890.

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* *Geschichte der Elektricität,* by Dr. Gustav Albrecht; Vienna and Leipzig, 1885.

bridge's,[*] and Mendenhall's,[†] have insistently presented their entertaining historical narrative to the reader. Among the technical treatises, Fleming's[‡] has essayed a partial history of electromagnetism; and not only are the accounts of Faraday's *Researches* available in their fulness of detail, but the reader has not far to go to find an almost equally elaborate statement of the experiments and conclusions of Joseph Henry.[§]

Having recognized these limited and partial accounts, the assertion may be ventured, that the essentially complete and philosophically considered history of the origins of the science of electromagnetism yet remains to be written. The apparent reason for the neglect of this labor must be ascribed to the rapid development and extensive application of electromagnetism, which have continually absorbed the available resources of the subject for their own advancement; and they have more or less diverted those fitted for the task from the scholarly labor of historical research, to the more popular and alluring field of experiment and the extension of the real or supposed boundaries of such phenomena.

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[*] *What Is Electricity?*, by Prof. John Trowbridge; International Science Series, New York.

[†] *A Century of Electricity*, by T. C. Mendenhall; Boston, 1887.

[‡] *The Alternate Current Transformer*, by J. A. Fleming; London, 1889–1892.

[§] *A Study of the Work of Faraday and Henry*, by Mary A. Henry; a serial in the *Electrical Engineer*, New York, 1892.

The labor of historical research in electromagnetism has, so far, been largely gratuitous, and this condition is likely to continue until the possibilities of new development are manifestly exhausted. Those who have been engrossed with the subject technically, or have been attracted to its study, have mainly been animated by commercial considerations, or personal ambitions. The commercial spirit is antagonistic toward historical research and unconcerned with its findings, except as they may further its own ends. The same stricture of selfishness, in many instances, may be charged against those who have sought in the pursuit of the subject a means for mere personal reputation. An exception to this statement must be taken in favor of the few who have been the real discoverers and promoters of this science.

The spirit of devotion for scholarly pleasure and the satisfaction of a normal intellectual interest, largely remains to be developed; and its development will probably follow upon the completion of the formulation of electromagnetism as an applied science. An intelligent and general interest in this subject, which is comparable with that which has been aroused by physics and chemistry, must first develop in order to support and encourage the labors of pure scholarship. After the mere wonderment over the mysterious and extraordinary phenomena of electromagnetism, and the exaggerated insistence on its manifold commercial applications, and similar transient conditions, have passed, this supporting and encouraging interest must certainly and rapidly develop.

CHAPTER II

THE RELATION OF THE SCIENTIFIC INVESTIGATOR TO HIS WORK

FROM the beginning of his study of historical science the reader is made aware of a singular and insinuating tendency, which persists throughout the nomenclature with which he is dealing: and this tendency is the designation of a fact or relation by the name of the individual, who has discovered the particular phenomenon or established its place in its appropriate science, or formulated one of its fundamental laws. Though this tendency does manifest itself in other than scientific directions, it is especially persistent and pronounced in scientific literature. The aggravated form which it here assumes is, to say the least, a reflection on those who originate such terms; and they suggest a paucity of imagination and the poverty of terse expressions. It is now Ohm's law, or Lenz's law that engages the attention of the reader; in the laboratory, the practice is almost universal, and what dignity is associated with the names of such apparatus as the Wheatstone bridge, the Kelvin balance, or the d'Arsonval galvanometer, is lessened by the commercial use of the name of some manufacturer which is suggestive neither of scholarship, nor intellectual superiority. The simplest experiments can scarcely be performed without, in this sense, thus materializing the names of past, or present worthies. Sooner or later, the thoughtful student must suggest the query, Why is the relation of current to electromotive force and resistance not termed the *current function*, rather than universally designated by the name of an individual?

In the mere recital of the facts and bearings of the scientific labors of an individual, these reflections might prove irrelevant. However, if the scientific work of an experimenter is to be studied in the light of the influence which the personality of the worker may exert upon his studies and labors, the inquiry will prove relevant and suggestive. The scientist is more than the bibliography of his writings and the catalogue of his experiments. He is, first, to be considered as an individual who has gained an expression for himself, to some degree, through his labors; and he is in a very real sense influenced and guided by the general tendencies which permeate, and the circumstances which surround, his chosen subject.

The labors of the scientist lack largely, or almost wholly, the power of the explicit expression of the individual. The architect may exercise his innate talent, and acquired technic, in the practice of his art or craft; and the building which he designs will be largely an expression of the man through his craft; and it will be truly an expression of the individual, for the design is dominated by his personal mode. Another could not have designed just such a structure; the particular building could not have been so designed, but for his existence. This, the individual expression which is shown in and through the structure, redeems the craft from the commercial commonplace, and attracts to its constructions a general and lasting interest. It is this very principle which imparts the abiding charm to all modes of art; and it is through this that the artist vitalizes his creations and embodies his very self in his work.

The case is radically different with the scientist. Though he may be possessed of marked genius and a strong individuality, his vocation is inadequate for the exhibition of his powers and the expression of the individual. One of high endowments may be somewhat more rapid in the play of the logical faculties than others, not so gifted; but this, in itself, is a trivial distinction, and is apt to lead to embarrassment in contrast with the accurate and painstaking findings and work of the industrious plodder.

Nature is impersonal, and a fact is not changed nor is a phenomenon influenced either by the personality and labors of the student, or the experiments of the investigator. In short, the processes of scientific activity constitute an instrument whose range is too limited for personal expression. It permits the display of ingenuity, invention and erudition, but is absolutely devoid of response toward creative impulse or power. It affords the imagination exercise in the interpretation of facts and relations, but is antagonistic toward its creative function. No play of the imagination can create a new region of phenomena, and constrain the known cognate facts and relations into the charmed order of the new realm.

Perhaps these considerations are, in a way, vaguely or even consciously realized by the most mechanical and artisan-like scientist; and it is such recognition which has induced a partially organized reparation by the wholly artificial means of assigning the origin of any branch of science in particular to that one, whose work in it is awarded the priority; very much as if that science were, in any wise, the product of the creative genius of the experimenter, rather than merely the outcome of his energy and application.

The history of science, however, fails to demonstrate that such a creative function exists between our knowledge of the facts of Nature and that one, who has brought them to our attention and comprehension. Almost invariably a fact of Nature is suggested by some fortunate happening, and the scope of the development of the associated phenomena being necessarily limited, one man as well as another is able to guide the course of the analytical or experimental development. Even if the subject-matter of the discovery, or development, is intellectual in part, and thus anticipatory, the mental processes that lead a certain investigator to successful results, are not in their nature different from those, which may be followed by other minds similarly trained and directed in attention, and equally skilled. An investigator, in other words, in the acquisition and extension of scientific knowledge, plays the single part of the recipient, and in no sense that of the creator; he plies his activities very much the same as the manual, or intellectual artisan, but at no stage is his work comparable with that of the artist.

In the selection of a name, technical or otherwise, a fundamental requirement is, that it shall be directly intelligible and convey an associated idea. When the name of an individual is assigned to a relation or fact, the term is not general and is remotely comprehended. Its first comprehension is through biography; then, some particular phase of scientific history, in order to come immediately upon the designation required. As an example of this practice, Fleming * calls the particular apparatus, "Henry coils," which were used characteristically by that investigator. It so happened that these coils were essentially those of the ordinary transformer. Complying with this pedantic custom, the transformer should be called, either the "Henry" or "Faraday coil," instead of being known by the intelligible name, the "alternating current transformer." Those who may observe such things attentively, will find that popular usage eventually departs from the pedantic and artificial, and shows a convincing preference for the intelligible; it will designate a fact by a

* Fleming, reference cited, volume i. page 157.

term that is descriptive and characteristic while refusing to accept a biographical designation. To insist on the latter, is merely to emphasize and belittle the pedantic in contrast with the sanity of the practical.

Ohm's law has been instanced in support of these observations. This law, which defines the magnitude of the current function, is merely a particular instance of a relation, which is a broad and general one in physics. Experiments with electrical currents could not have proceeded far until they had revealed a necessity for such a law, and eventually established it. The individual investigator, who might demonstrate and phrase such a law, would merely be the first one in respect to priority. Whether, then, the circumstance of having been the first historically should justify encumbering the spoken and written phrasing of such a law with the name of an individual, is obviously a matter which has sufficient significance to be worthy of some consideration, however self-interest and policy may dictate practice.

This law was demonstrated by Ohm* in 1827, but the relation was partially anticipated by Henry Cavendish† in 1781, and perhaps by others; and it was certainly independently discovered by Joseph Henry‡ a few years subsequent to Ohm's neglected announcement. So, whether Ohm had formulated this relation or not, the law assuredly would have been established prior to 1835.

Recent instances of the inevitable tendency in discovery have been even more convincing. The name of Hertz has thus far been inseparably connected with the peculiar properties shown by a stressed field surrounding an electrical charge whose equilibrium has been disturbed. The classical researches of

*G. S. Ohm, *Die galvanische Kette mathematisch bearbeitet*; Berlin, 1827.

† *Cavendish Researches*, articles 574–5, 629, 686.

‡ *Electrical Engineer*, vol. xvii, 1894, page 297.

Hertz * began at the suggestion of Von Helmholtz in 1879, and were later resumed in 1886. Yet these phenomena were anticipated, and had been obscurely recognized, long before this; and had Hertz failed to have developed their definition and precise knowledge, the increased attention attracted to alternating current phenomena would very quickly have led investigators to the full experimental demonstration of electromagnetic waves. Had nothing else done so, the protection of long power lines against lightning would alone have led to the discovery of the mechanical properties of the stressed field.

The beginnings of the science of electromagnetism strikingly exhibit this phase of the impersonal character of scientific work; and, if the development of this science is broadly considered, it will show convincingly how slight is the

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* *Miscellaneous Papers*, by Heinrich Hertz, English translation by Jones and Schott; London, 1896.

influence of the supposed genius of the investigator. In the history of this subject, there is found a record of at least several persons making the same discoveries independently, and endeavoring to derive the same laws from the like trend and relations of the involved phenomena.

Such occurrences suggest the reflection that the spirit of a new advancement in thought, or a generalization, may be existing in many minds, and be a common possession long before it is fully grasped and realized ; it appears now in one place and, again, in another without an apparent common stimulus which would be adequate to produce such an unity of affection. At the most, the investigator, even possessed of the highest talent, can but anticipate the labors of others by a longer or shorter period, can exhibit a little more enterprise and initiative than his fellows ; but the facts and laws of Nature will ultimately be possessed, whether one man or another discovers them. The relation of the investigator to the development of science is rather that of the skilled and learned workman; and, while honoring the labors of the investigator, and fully acknowledging his essential connection with the advancement of science, it must be conceded, that the real scope and task of genius is not in experiment and investigation; rather he is the genius, *who builds upon facts the philosophical system of their laws.* In the subject of electromagnetism it is James Clerk Maxwell who does this so voluminously for the researches especially of Faraday,* and Franz Neumann for the results of the work of Lenz.†

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* *Electricity and Magnetism*, vols. i. and ii., by James Clerk Maxwell.

† *Die mathematischen Gesetze der inducirten elektrischen Ströme*, by Franz Neumann, 1845; Leipzig, 1889.

CHAPTER III

THE DISCOVERIES OF FARADAY AND HENRY

AS the work of Lenz has not generally been accredited with pronounced originality, and influence upon the earlier history of electromagnetism, it is appropriate to review this with sufficient detail to assign the contributions of Lenz their importance and historical place. A mere recital of his experiments and conclusions would by no means be convincing and historically adequate.

Lenz was not an investigator working out his experiments along wholly original lines; he was guided and influenced by the publication of the discoveries of other scientists, and directed by the scientific tendencies with which he was in contact. Among these earlier investigators the experiments of Faraday, to a large extent, influenced the researches of Lenz and supplied him with the initiative for his own labors.

The opinion of an historical event and the acceptance of it, which were current and popular in its immediate time, rarely prove adequate and truthful to the later and matured consideration which is free from prejudice and prepossession. No exception to this tendency is found in the history of electromagnetism; the narrative of the discoveries of the phenomena and principles of this science has scarcely passed beyond the state of primitive acceptance, for it is yet almost exclusively occupied and predominated by the classical work of Faraday.

To acquire a matured and definitive estimate of this event in science, a knowledge must be had of the times in which it occurred, the contemporaneous tendencies, and the particular discoveries and researches of those scientists who essentially contributed to the movement.

The prime discoveries and the early development of electromagnetism have commonly been ascribed to Faraday; but there is another scientist whose just claims for recognition have, until recently, been overlooked. It is now clearly recognized that Henry is entitled to an essentially equal place historically with Faraday; and, though Lenz was little influenced, if at all, by the discoveries of Henry, yet the definitive estimate of this subject requires the ultimate association of the work of Faraday, Henry, and Lenz.

In the chronological sequence of discovery and research, the work of Henry generally takes precedence over that of the others, although its delayed publication deprived it of much of its influence and prestige. Of the three men, Henry's advantages and position were the least favorable. Henry, during this eventful period, was a professor of mathematics and natural philosophy in the humble Albany Academy, New York; Faraday was a professor in the Royal Institution, London; and Lenz was professor of physics in the University of St. Petersburg. It is needless to insist further on the contrast of opportunities!

For the full appreciation of such important events, which have made an epoch in science, something more is needed than the mention of their day and year in the succession of the historical data of discovery and development. It is necessary to revive something of the scientific atmosphere and feeling of their day; and to place the mind in sympathetic touch with the master-thoughts of those times in which occurred the development of electricity from magnetism. To the contemporaries of Faraday, Henry, and Lenz this conception was as charged with enthusiasm and expectation as the present-day thought of the direct production of electricity from carbon, or the refinements of the ether waves.

Perhaps there is no more vital and truthful means for gaining an intimate appreciation of those days of remarkable discoveries, than the reading of the simple narratives of those experiments by which Faraday, and Henry, more especially, announced publicly the assured demonstration of the scientific expectation of those years. As these scientists have taken their readers so largely into their confidence, when describing their experiments and stating their conclusions, this narrative is better told by quoting at considerable length from the original papers and memoirs. The account by Joseph Henry is given first, not that preference is accorded him from national prejudice, but he is more frank and generous in the recital.

The famous contribution of Henry begins: "Although the discoveries of Oersted, Arago, and others have placed the intimate connection of electricity and magnetism in a most striking point of view, and although the theory of Ampère has referred all the phenomena of both these departments of science to the same general laws, yet until lately one thing remained to be proved by experiment in order more fully to establish their identity, namely, the possibility of producing electrical effects from magnetism. It is well known that surprising magnetic results can readily be obtained from electricity, and at first sight it might be supposed that electrical effects could with equal facility be produced from magnetism; but such has not been found to be the case, for although the experiment has often been attempted, it has nearly as often failed.

"It early occurred to me that if galvanic magnetism, on my plan (see vol. xix. *Silliman's Journal*, page 400), were substituted for ordinary magnets, in researches of this kind more success might be expected. Beside their great power, these magnets possess other properties, which render them important instruments in the hands of the experimenter — their polarity can be instantaneously reversed, and their magnetism suddenly destroyed or called into full action, according as the occasion may require.

"With this view, I commenced last August (1832) the construction of a much larger galvanic magnet than to my knowledge had before been attempted, and also made preparations for a series of experiments with it on a large scale in reference to the production of electricity from magnetism. I was, however, at the time accidentally interrupted in the prosecution of these experiments and have not been able to resume them, until within the last few weeks, and then on a much smaller scale than was at first intended." *

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* This article was printed in the appendix to *Silliman's Journal,* vol. xxii. October, 1832; the title was,

Professor Henry then records the statement that he had read in the 117th number of the *Library of Universal Knowledge*, "that the result so long sought after has at length been found by Mr. Faraday, of the Royal Institution." He then quotes in full the announcement from the April number of the *Philosophical Magazine*, under date of February 17, which is subsequently given.† He then continues:

"Before having any knowledge of the method given in [this] account, I had succeeded in producing electrical effects in the following manner, which differs from that employed by Mr. Faraday, and which appears to me to develop some new and interesting facts. A piece of copper wire about thirty feet long and covered with elastic varnish, was closely coiled around the middle of the soft iron

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"On the Production of Currents and Sparks of Electricity from Magnetism."

† Consult page 44.

armature of the galvanic magnet (described in vol. xix. *Silliman's Journal*). The armature thus formed with wire was placed in its proper position across the ends of the galvanic magnet, and there fastened so that no motion could take place. The two projecting ends of the helix were dipped into cups of mercury and there connected to a distant galvanometer by means of two copper wires, each about forty feet long. This arrangement being completed, I stationed myself near the galvanometer and directed an assistant at a given word to immerse suddenly in a vessel of dilute acid the galvanic battery attached to the magnet. At the instant of immersion the north end of the needle was deflected 30° to the west, indicating a current of electricity from the helix surrounding the armature. The effect, however, appeared only as a simple impulse, for the needle, after a few oscillations, resumed its former undisturbed position in the magnetic meridian, although the galvanic action of the battery, and consequently the magnetic power, was still continued. I was, however, much surprised to see the needle suddenly deflected from a state of rest to about 20° to the east, or in a contrary direction when the battery was withdrawn from the acid, and again deflected to the west when it was reimmersed."

On the first impression it may seem singular that the announcement of Faraday's success was so long delayed in coming to the attention of Henry; but it must be remembered that the transmission of news between Europe and America was slow and uncertain, and, beside, Henry was then a professor in a small school, a mere academy, in the remote town of Albany. Overburdened with work during the school-year he was compelled to postpone his investigations to the vacation intervals. As was his custom, he was in this year, 1832, deeply engrossed with important experiments during the summer vacation; and these, for lack of better accommodations, were performed in his class room improvised into a temporary laboratory.

Perhaps through his very preoccupation with his own investigations, but no doubt more through the slow diffusion of news to a place so remote from a large center, it is possible to account for the circumstance, that the report of Faraday's success did not come to the notice of Henry until he had achieved an independent discovery of the same phenomena. The important historical fact is, that Henry independently made the fundamental discovery in electromagnetism before learning of the announcement of Faraday's demonstration published in the *Philosophical Magazine.* This laconic announcement, to which Henry referred in his communication, occurs in connection with a report of the proceedings of the Royal Institution; it reads:

"February 17. *The Conversion of Magnetism into Electricity.* — This long-wished-for result has at length been obtained by Mr. Faraday. If two wires, *A* and *B*, be placed side by side, but not in contact, and a voltaic current be passed through *A*, there is instantly a current produced by induction in *B*, in the opposite direction. Although the principal current in *A* be continued, still, the secondary current in *B* is not found to accompany it, for it ceases after the first moment." *

It is interesting to turn aside from these formal communications in attempt to realize something of the general interest which such discoveries aroused, and the enthusiasm with which they were repeated and varied. Perhaps the enthusiastic interest awakened by Faraday's first announcement of the success of his experiments was relatively greater than that so recently excited by the Röntgen-ray discoveries: the occasion of the former was more impressive, and the final results to accrue from them were of far greater importance; yet the latter were apparently more popularly received, to judge from the hysterical and notorious advertising which they achieved through the newspapers, and the efforts which many persons exerted to lift themselves into prominence through them.

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* *Philosophical Magazine*, vol. xi. April, 1832, page 300; also reprinted in the *American Journal of Science* (*Silliman's Journal*), volume xxii. October, 1832, page 386.

Just what the popular reception of Faraday's discoveries may have been, is more vividly realized through the correspondence of certain interested persons, who were repeating these experiments, than through the formal accounts read before the learned societies.

In this country Professor A. D. Bache of the University of Pennsylvania contributed early in this period of discovery an interesting account of his electromagnetic experiments to the *Journal of the*

Franklin Institute.[*] His contribution was prefaced with several extracts from private letters[†] written from London during these noteworthy months; the first letter[‡] is the more interesting in this connection. It states:

"You may have heard of Faraday's curious discovery in electromagnetism. He has succeeded in obtaining from a magnet a spark resembling the electric spark." What follows relates particularly to Faraday's wheel, which he presently contrived after making his initial discovery.

"The apparatus consisted of a copper plate," the letter continued, "like an electrical machine, and made to revolve between the poles of a large magnet. The plate used was about one foot in diameter, and one-eighth of an inch in thickness. The rim of the wheel was amalgamated, as well as a ring around the axis. Two pieces of copper, shaped to fit, for two or three inches, the amalgamated ring and the circle, were attached to either wire of a galvanometer. These pieces of copper being applied to the wheel, and the latter turned, the needle of the galvanometer vibrates: the amount of vibration may be increased by alternately touching and removing one of the plates. The copper plate which touches the rim should be between the poles of the magnet.

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[*] "Electro-Magnetic Experiments," volume x. July, 1832, page 66.

[†] These letters were written by J. Saxton, to Isaiah Lukens, Philadelphia. Isaiah Lukens was the first vice-president of the Franklin Institute, Philadelphia.

[‡] Dated, "London, April 14, 1832."

"I have made this experiment in a different way and succeeded satisfactorily. The method was as follows. A coil of wire wrapped with silk, similar to that used in the galvanometer, was attached by the ends to the wires of the galvanometer. On passing this roll backward and forward upon one of the poles of a horse-shoe magnet, or placing it upon and removing it from either pole, I have made the needle of the galvanometer spin round rapidly."

A romantic interest will always be associated with Henry's crucial experiments, whether he will ultimately be admitted to equal honors with Faraday or not, as an original discoverer in one of the most important branches of science. His work was heroic in the face of almost insuperable discouragements; and the fine qualities of mind and high endowments, which his stern circumstances but more clearly revealed, must win for him continued regard and real veneration.

It is unfortunate that eventualities should present two so illustrious and remarkable men, as claimants for the same high honor. Henry and Faraday were singularly alike in character and endowments: they were men whose integrity and amiable qualities were as conspicuous as their scientific abilities.

Should the honor of discovery be given to the one, rather than awarded in common to both, it would be unfortunate for science and an injustice would be done to the general cause of progress.

It is well to consider, even briefly, the lesson which this epoch in science may have for progress at large. The lesson unmistakably shows the fallacy of over-accenting the individual, and singling out one person to be the recipient of an honor or prize. In this instance, broad discernment alone can arrive at a just and true estimate, and one which may be ultimately accepted.

What is apparent in the broader view, is, that the spirit of this important discovery was at work generally among scientific men at this time. It is not a question, then, of the priority of a few days, weeks, or even months, so much as it is a concern how this diffused idea took on a material form. It should be accepted as a singular fact, that two widely separated scientists should independently and simultaneously be making the same discoveries ; and it seems only fitting, that each scientist should have awarded him the honor for his personal work alone. It is our mistaken zeal which attributes to an individual, a profound influence upon his own time. In this specific case the tendency was general, and Faraday and Henry, each in his own degree, inspired and directed this tendency to splendid material results.

Approaching the recital of Faraday's work, there is found less of the heroic and interesting about it; he has not introduced so much of the personal element into the formal narrative of his experiments.* Faraday is more given to the scientific formalities of his theme, and is, perhaps, less enthusiastic ; though

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*Read November 24, 1831, and published in the *Philosophical Transactions* of the Royal Society, part i. 1832, pages 125–163 ; also, volume i. *Experimental Researches*, by Michael Faraday, pages 1–41.

it is evident that he was fully aware of the importance of the new phenomena. He prefaces his narrative with lengthy philosophical observations in which he laboriously defines the sense in which he proposes to use the term, "induction." After briefly referring to some known phenomena in magnetism and static electricity, he remarks:

"These considerations, with their consequence, the hope of obtaining electricity from ordinary magnetism, have stimulated me at various times to investigate experimentally the inductive effect of electric currents. I lately arrived at positive results, and not only had my hopes fulfilled, but obtained a key which appeared to me to open out a full explanation of Arago's magnetic phenomena, and also to discover a new state, which may probably have great influence in some of the most important effects of electric currents."

Faraday's way to success in endeavors

to establish his preconceptions and hopes lay through many failures and partial results; while it may be recalled that Henry brilliantly proved his grasp on the same thought by one simple and decisive experiment, the like of which, it will be seen, Faraday was led to in the end.

It has previously been observed that the thought of the generation of electricity from magnetism had materialized itself into a practical experimental idea, which was similarly held, at least, by many scientists. This idea was, in short, that the magnetism which accompanied a voltaic current in a wire *A*, by induction should set up a similar current in a wire *B*, placed parallel with it. And we now understand that the reason for the failure of these early experimenters did not proceed from false premises, but, rather, from the circumstance that the galvanometers then used were crude and insensitive instruments. The criterion for their success was not some far-seeing grasp of a vague principle, but some mechanism sufficiently powerful to make the phenomenon of induction evident through the crudeness of the experimental apparatus, then in use. Faraday states this, by tacit inference, in a "corrective chronological note," written in April, 1832:*

"I experimented on this subject several years ago, and have published results.† The following is, also, an extract from my note-book, dated November 28, 1825: 'Experiments on induction by connecting wire of voltaic battery; a battery of four troughs, ten pairs of plates, each arranged side by side—the poles connected by a wire about four feet long, parallel to which was another similar wire separated from it only by two thicknesses of paper, the ends of which were attached to a galvanometer—exhibited no action, etc., could not in any way render any induction evident from the connecting wire.' The cause of failure at that time, is now [April, 1832] evident."

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* *Experimental Researches*, page 41.

† *Quarterly Journal of Science*, July, 1825, page 338.

Faraday's crucial experiments can only be given in brief notes, although their elegant logical arrangement in his paper would justify a lengthy quotation. The more important experiments were :

(1) About a cylinder of wood, some twenty-six feet of copper wire, 0.05 inch diameter, was wound alternately with twine for insulation. (It is worth while to notice that Faraday had not come upon the idea of insulating the wire itself, as Henry had done). This helix was then covered with calico, and a second helix was similarly wound upon it; and so on, until twelve such coils had been wound. Two separate circuits were formed through these coils by connecting the alternate helices in series. Two wires, each 155 feet in length, were thus coiled into parallel and similarly

directed electrical circuits; though we are not informed of the number of turns in the compound helix. One circuit was connected with a galvanometer, and the other with a powerful voltaic battery. It is recorded, "that not the slightest sensible deflection of the galvanometer could be observed."

(2) "A similar compound helix, consisting of six lengths of copper and six of soft iron wire, was constructed. The resulting iron helix contained 214 feet of wire, and the resulting copper helix, 208 feet; but, whether the current from the trough was passed through the copper or the iron helix, no effect upon the other could be perceived at the galvanometer." This experiment shows especially how insensitive Faraday's galvanometer must have been, otherwise one can not understand why he failed to obtain some noticeable deflection.

(3) "Two hundred and three feet of copper wire in one length were coiled

around a large block of wood; other 203 feet of similar wire were interposed as a spiral between the turns of the first coil, and metallic contact everywhere was prevented by twine. One of these helices was connected with a galvanometer, and the other with a battery of 100 pairs of plates, four inches square, with double coppers and well charged. When the contact was made, there was a sudden and very slight effect at the galvanometer, and there was, also, a similar slight effect when the contact with the battery was broken." Partial success in this experiment seems to have been obtained as a consequence of the excessively powerful battery used, which was several times stronger than that employed in the first test.

After having established the existence of this "Volta-electric induction," as he termed it, Faraday proceeded to the further task of "evolution of electricity from magnetism."

(4) This experiment is so well known and so similar to that already enlarged upon in connection with the work of Henry, that it will be very briefly summarized. "A welded ring was made of soft, round bar iron, seven-eighths of an inch in thickness, and the ring being six inches in external diameter." Two compound helices were wound, as above, on the ring, opposite each other in position. "The helix *B* was connected by copper wires with a galvanometer, three feet from the ring." The helix *A* was "connected with a battery of ten pairs of plates, four inches square. . . . . . The galvanometer was immediately affected, and to a degree far beyond what has been described when, with a battery of tenfold power, helices *without iron* were used; but, though the contact was continued, the effect was not permanent, for the needle soon came to rest in its natural position, as if quite indifferent to the attached electromagnetic arrangement.

Upon breaking the contact with the battery, the needle was again powerfully deflected, but in the direction contrary to that induced in the first instance."

Immediately upon the discovery of the induced current, Faraday seems to have sought zealously for a physical or mechanical explanation of its occurrence. He expanded some older and cruder conceptions into the very happy one, of lines of magnetic force. No finer exercise of the philosophical imagination has ever been made than that, which led him to frame the notable idea of the "magnetic curves," as he named them.

Although Faraday was not a practised artisan in the use of mathematical analysis, in this instance he was guided by the real genius of this science to conceive of lines of magnetic induction working in a medium. These lines Maxwell found upon his analysis, to be "mathematically true."* In another place he pays this significant and just tribute to Faraday's methods, that he is "convinced of their superiority." *

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* *Electricity and Magnetism*; 3d. ed. vol. ii. page 28.

It is not only possible, but highly probable, that our advanced state of experiment and mathematical analysis has not yet grasped the finer mechanical significance of the physical state for which the *line of force* is a name.

The conception of these magnetic curves is the most characteristic and individual contribution which Faraday has made to the science of electromagnetism; and its description is the most marked detail of this historical paper, when the reader recalls the contemporaneous work of Henry.

The recital of the origins of electromagnetic induction has now reached that point in time, when the work of Lenz begins to have an influence upon further discoveries and developments within the new science. It has prepared the way, possibly, for a clear estimate of the relation and value of the work of Lenz to the more illustrious labors of Faraday and Henry.

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* "I have since become more convinced of the superiority of methods akin to those of Faraday, and have therefore adopted them." From the Author's Preface, *Elementary Treatise on Electricity*, by James Clerk Maxwell, edited by William Garnett; Oxford, 1881.

Some impromptu lines of Herbert Mayo* suggest themselves in concluding this historical study, although they are more applicable to the experiments of Henry:—

> "Around the magnet, Faraday
> Conceived voltaic lightnings play;
> Yet could not draw them from the wire!
> He learned a lesson from the heart:
> 'T is when we meet, 't is when we part,
> Breaks forth electric fire!"

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* Altered from page 200, *Michael Faraday*, by J. H. Gladstone; New York, 1872.

PART TWO

THE EXPERIMENTS AND DISCOVERIES OF LENZ IN MAGNETISM AND ELECTRICITY

CHAPTER IV

EARLY EXPERIMENTS AND CONTRIBUTIONS

HEINRICH Friedrich Emil Lenz was a voluminous writer, as well as an indefatigable investigator and experimenter. In addition to his books, the Royal Society's *Catalogue of Scientific Papers* credits him with a list of some fifty monographs and papers. His earlier writings were discursive, and a variety of subjects engaged his attention. Upon his appointment to a professorship of physics in the University of St. Petersburg, this preparatory scientific work ceased; and Lenz then entered upon a period of maturing powers, which were zealously devoted to experiments and researches in electricity and magnetism.

But little biographical information is available concerning Lenz, for his residence was remote from the intellectual centers of Europe. He was compelled to seek abroad the means for the wider publication of his papers; they usually appeared in German periodicals. However there may be found a less evident, but more potent reason for the obscurity of the personal records of his life, in the practice of scientists themselves: they, like other skilled artisans, become absorbed by the deed and the fact to the neglect of personal records beyond their scientific papers, and the account of their experiments.

An additional reason may be, that society at large, out of requital for the scientist having eschewed and neglected the humanities, forbears to consider his own life as a human document worthy of preservation. Commonly the author of the most insignificant literary work takes occasion to leave a sufficient record of his life to show what the manner of the man was; but, from the silence of social records, one must usually take for granted that the scientist was a man fulfilling, at least, the ordinary round of affairs.

In these preparatory years before he received the responsible appointment from the University, there are creditable evidences that Lenz was unusually endowed, and was developing a character to be marked by energy and enterprise. During this period he contributed some six papers to the German periodicals and the publications of the St. Petersburg Academy of Science.

Of themselves these papers characterize his mind to have been inquiring and eager; energetic, rather than philosophical; and apt to be absorbed by his near environment, or the dominant idea of the moment. By the very titles of these papers he affords an ingenuous introduction to the intellectual qualities of the man. In their chronological order these titles show a progress from the natural to the physical sciences; their order in the *Catalogue* is:

I. "On the Temperature and the Salt-Content of the Water of the Ocean at Different Depths."*

II. "Report on a Journey to Baku;" the contents are geological in import.

III. This paper supports the pretentious caption, "Physical Observations Made during a Voyage around the World, with Captain von Kotzebue, in the Years, 1823–6."

IV. A second contribution on a previous subject has for its title, "On the Comparative Quantity of Salt Contained in the Waters of the Ocean."

V. "On the Influence upon the Movements of the Beam of a Balance, Due to Bodies at Different Temperatures in its Neighborhood."

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*The citations for this series of papers are given in a subsequent chapter which deals at length with the bibliography of Lenz.

VI. "On the Variations in Height which the Surface of the Caspian Sea Has Suffered to April [*sic!*] of the Year, 1830."

It is at this time that Lenz made his first significant contribution of general scientific interest; it is the seventh in the list of his papers, and places him at once among the eminent investigators of this eventful period. Our interest in the work of Lenz is more especially aroused by this contribution; and this memoir challenges favorable comparison with the eminent contemporary papers on its subject-matter. Exhaustive and thorough in character, the memoir is convincing, and ranks its author among the foremost investigators of the new science.

It bore the formal title, "On the Laws which Govern the Action of a Magnet upon a Spiral, when it is suddenly Approached toward, or Withdrawn from it; and the most Advantageous Method for Constructing Spirals for Magneto-

Electrical Purposes."[*] This important paper was read on November 7, 1832, before the Academy of Science at St. Petersburg; and it is significant in its date, when a comparison is made with similar contributions by Henry,[†] and Faraday.[‡]

In this paper Lenz frankly mentions Faraday's published experiments, and states that he promptly repeated the principal ones. It is a difficult task to judge this paper without prejudice, and discriminate the influence of suggestion that was received on repeating Faraday's work from the real initiative, which Lenz had brought to his researches. Upon the critical reading of his own statements, it becomes apparent to what extent he verified the work of another, and how far he is entitled to be considered an original discoverer of some of the various phases of the induction of electricity from the magnetic field.

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[*] An English translation by W. Francis was given in Taylor's *Scientific Memoirs*, vol. i. 1837, page 608.

[†] Published in October, 1832.

[‡] Read on February 17, 1832.

Quoting from the English translation of this paper, Lenz introduces the recital of his early experiments with the explanation: "From the great interest which the late discoveries of Faraday in the field of electromagnetism must awaken in all the natural philosophers of Europe, it is to be expected that we will soon receive many and various explanations [to account for them]. Up to the present moment we, here in the North, are only acquainted with the papers of Becquerel, Ampère, Nobili, Antinori, and Pohl; and, as none of these authors has occupied himself with that branch of the subject to which I have directed my particular attention, I hasten to make known, as quickly as possible, these contributions to the science of electromagnetism."

After having successfully repeated the experiments of Faraday, Lenz applied himself to investigations which might determine how the phenomena of the magnetic action on a spiral, suddenly approached toward, or removed from a magnet, could most readily and powerfully be produced. In his attitude when attacking these studies, Lenz showed the modern spirit of research through his desire to ascertain magnitudes and establish quantitative relations. His work is more enduring in this respect, and is superior to the more qualitative research of Henry and Faraday.

A comparison of the technical writings of European and English-speaking scientists, shows a racial distinction which predominates over individuality. Until this period, and for a decade or two succeeding, the work of English-speaking scientists is qualitative and aggressive, is marked by resource and invention, and shows the influence of strong and vivid imagination. Contemporary Continental literature is prevailingly analytical, and is marked by the extended use of the more mathematical methods. It is slow, tedious and pervaded by the influence of the schoolmen; but its results were fundamental, and have been enduring.

While planning his experiments Lenz foresaw, that he must determine in what respects the characteristics of the "electromotive spiral," as he called it, exerted an influence upon the phenomena. The characteristics which he anticipated and studied, were:

(1) The number of convolutions in the spiral;
(2) The breadth,
(3) The thickness, and
(4) The substance of the convolutions.

As more than usual interest attaches to the design of the galvanometer which was employed in these researches, it is essential to learn that it was constructed with Nobili's double, or astatic needle; and was wound with seventy-four turns of wire, 0.025 inch diameter. Describing other details of the apparatus, the paper states: "I wound the electromotive wire about a soft iron cylinder, which served as an armature, and was filed smooth and flat at those places, where it was laid on the poles of the magnet. As the removal of the armature can be performed in a more certain, prompt and uniform manner than the placing of it on the poles, I have, in all my following experiments, given the results which were obtained by pulling away the armature, or by the sudden removal of magnetism from the iron."

With these elaborate preparations, he proposed to investigate the various factors in the induction of electrical currents by magnets. He shows throughout the discussions of this paper, that he was thoroughly familiar with the recent laws of Ohm for the electrical circuit, and had grasped the full physical significance of their factors, and knew how to combine them quantitatively. He also intimates that he had discovered these laws for himself before learning of Ohm's work.*

From the results of his experiments, Lenz derived a number of generalizations. In the first of these he finds,

"The electromotive power, which a magnet produces in a spiral with convolutions of equal magnitude, and with a wire of equal thickness and like substance, is directly proportional to the number of its convolutions." It is very interesting to find so early a mention of this fundamental law, which from wide and general use has lost all trace of its original recognition.

In the accepted phraseology of this relation, when a coil of n turns, or convolutions, is moved in a magnetic field, or the field relative to it is changed in magnitude by N lines of force, there is an electromotive force e induced in it which is defined in C. G. S. units by the expression,

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* Reference cited, page 613; also compare, in this connection, a statement on page 29 of this book.

$$e = -n\frac{dN}{dt}.$$

In this generalization only comparative magnitudes were recognized by Lenz. Doubtless the conception of an absolute determination occurred to him, but magnetometry had not yet been developed; and, from lack of all knowledge of the magnitude of the magnetic field which he employed, the influence of the time-rate of change in the strength of the field escaped him.

For the second series of experiments, "I wound," he states, describing the apparatus, "the copper wire in six convolutions around a wooden wheel twenty-eight inches in diameter, and placed the wheel on the iron cylinder. After having completed the experiment, I wound six convolutions of the same wire about the same iron cylinder." Nearly identical electromotive forces were obtained with each arrangement; and the experiments led him to the conclusion,

"The electromotive power which the magnetism produces in the surrounding spiral, is the same for every magnitude of the convolutions."

For the third series of experiments three separate coils were made, each with ten convolutions, whose wires were of different diameter. The length of the wire in each coil was thirty-three feet; and, since the wires were short and relatively of large section, their resistance seems to have been negligible for these tests. The coils were tried successively on the same cylindrical iron core, with the result that the electromotive force induced was practically constant for the three coils. From these experiments was derived the third of this important series of generalizations,

"The electromotive power produced in the spirals by the magnet, remains the same for every thickness of the wire, or is independent of it."

The fourth deduction, which he derived from these tests, is of great philosophical interest. Modern research has added practically nothing of importance to the finding of Lenz; and, in the wide application of the preceding principles, this last conclusion is overlooked, or regarded as obvious. For the investigator, who is seeking for a physical explanation of the nature of magnetism, this generalization is most significant. By simple and conclusive tests, Lenz proved very clearly, that "the electromotive power, which the magnet produces in spirals of wire of different substances, under like conditions, is the same for all substances."

From one point of view this conclusion was rigidly correct: while it holds true, as well, for coils of ferro-magnetic windings, whose permeability is greater than unity, such materials affect the value of the factor dN and mask the final result; though this refinement Lenz seemingly was unable to detect.

His own remarks will be placed in evidence to show how thoroughly Lenz had then grasped the principles of the electrical resistance of conducting circuits and substances. While describing the experiments leading to this conclusion, he says:

"Nobili and Antinori,* in their first paper on the electrical phenomena produced by the magnet, have already determined the order in which four different metals are adapted to produce the electric current from magnetism. They arrange these in the following order: copper, iron, antimony, and bismuth."

"It is particularly striking," Lenz then continues, "that the order is the same as that which these metals occupy, also, in reference to their capacity of conducting electricity: and the idea suddenly occurred to me, whether the electromotive power of the spirals did not remain the same in all metals; and whether the stronger current in the one metal did not arise from its being a better conductor of electricity than the others. With this in view, I examined four metals: copper, iron, platinum, and brass."

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* Poggendorff's *Annalen der Physik und Chemie,* number iii. 1832.

His method of experiment was both simple and conclusive: spirals identical in form were made of the several metals to be investigated; and these were tested by pairing them at a time, and connecting the two in series into a single circuit whose free ends were joined by copper lead wires to a galvanometer.

One spiral of the pair was first placed on the soft iron armature of the magnet; and then the other was similarly tested. During the experiments no change thus occurred in the resistance of the circuit; and, providing there was no variation in the electromagnetic conditions for the coils, an equality in the inductive effects should have been obtained for the two tests.

Among the circuits containing the sets of coils, an allowance was made for the internal resistance of each pair; and, upon calculating the electromotive force in units for each coil, it was found that these values agreed remarkably well for the entire series of tests. Among the refinements of these experiments, were the pains taken to avoid thermo-electric action at the connections of the different metals; for it is to be remembered that the thermo-electric generation of an electromotive force had been discovered by Seebeck,* ten years before Faraday and Henry had demonstrated the induction of an electromotive force by magnetism. Finally, Lenz derived a law which he thus phrases,

"The electromotive power which the magnet produces in spirals of wire of different substances, but in every other respect placed in exactly the same conditions, is completely identical for all these substances."

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* Poggendorff's *Annalen*, Band vi.

From our greater attainments, wider knowledge, and perfected appliances, we look back upon the work of the noted investigators, and call it deservedly great, and we regard their labors with scholarly admiration; yet, usually, from ignorance of the mental attitude of these scientists toward their researches, we imagine their intellectual grasp upon them to have been exceptionally strong, and extraordinary, when often it was not above the commonplace, and, at times, puerile. It is the usual practice to award prestige on tradition, rather than accord it from the conviction of knowledge! How the early scientists were obscure in their vision, and hesitated in uncertainty; how, as a result, they made a significance of the trifling and immaterial, Lenz may again be quoted in illustration. Still writing of his experiments with the same electromotive spirals, he adds:

"The conducting wires were connected with the ends of this spiral only by a single twist of the wires; the result of four readings with the connection in circuit amounted to 36.8°; upon this, the same connection was made by twisting the ends of the wires ten times around one another, as tightly as possible; the deviation again amounted to 36.8°. I finally pressed the last connections as tightly as possible with a pair of pincers, so that they were very much flattened; the deviation was 36.75°. We may, therefore, consider the connections made by tightly twisting the wires ten times around one another, as quite sufficient."

In the development of the discussion of his experiments, Lenz analytically investigated the magnitude of the current induced; which was done from the view he then held of the sine galvanometer as an instrument for measuring an electrical current. It must be taken into consideration that galvanometry, up to this time, had been pursued only by methods of static deflections. No occasion had arisen for measuring impulsive effects by ballistic deflections; the attention of scientists had scarcely been directed to their significance. One year later, Gauss was to publish his early paper on magnetometry in the determination of the magnetic intensity of the earth's field.*

Lenz pursued his analytical discussion with some show of skill; but, from lack of careful discrimination between static and ballistic deflections, the results which he obtained have a considerable intrinsic error. This did not affect these quantities relatively, except in their absolute value; since the deflections were usually of similar magnitude. It is an indication of the thoroughness and painstaking care with which this paper was prepared, that its results are corrected by the methods of least squares; yet this, in our eyes, is an unwarranted refinement, and far in excess of the instrumental accuracy of the work.

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* Poggendorff's *Annalen*, Band xxviii. 1833.

In the final conclusion to this classical paper, the conditions for the maximum electromotive force which could be produced with these apparatus, were sought by investigating the algebraical relations derived from the data of the experiments. The spirit and method of this analysis is formal and academical; and, perhaps, these processes would be called crude in comparison with our prevalent standards. The expressions would have been more concisely phrased had the analysis been made by differential processes; though Lenz proves himself to have been familiar with these by their use in later papers.

In common with contemporary writings, the reasoning of this portion of the memoir is pervaded by bald assumption. Some of the terms which Lenz assumed were mere philosophical conceits; yet, by these means, he arrives at some conclusions which appear to have been satisfactory at that time, though they are devoid of interest to the present-day reader.

The contemporary researches, and frequently identical results of the leading scientists who laid the foundations for the science of electromagnetism, have now been recorded and placed in their due order. Of these discoverers, Faraday and Henry are entitled to equal and divided honors for their initiative work. In comparison with these, the part which Lenz played in this movement was that of assigning magnitudes and verifying phenomena; and, later on, formulating an important generalization.

Of the three investigators, Faraday showed preëminent genius in seeking after the causes of the new phenomena, and developing the useful working hypothesis of tubes of force. Both Henry and Faraday neglected to attempt any mathematical formulation and deduction from their experiments: with Faraday this resulted from a lack of working facility with mathematics; and Henry, evidently, was not fully accustomed to the attitude of mathematical natural philosophy, and its academical acceptance.

Again, of the three, the work of Lenz best stands comparison with modern methods; and among them he is preëminently the precise and analytical scientist seeking for relations and magnitudes. His work makes the impression, however, that its author was singularly devoid of originality; that he was empirical, rather than ingenious; and that he undertook his experiments in the spirit of the skilled workman, who carefully and methodically examines every phase of his task, that nothing may escape him.

While much of this is commendable, when unmarked by superior qualities, it does not prove that the scientist, whom it characterizes, has been inspired by a high degree of the philosophical insight and spirit.

When several of his subsequent papers are taken into the present consideration, it is impressed that Lenz displayed a remarkably clear conception of the momentary character of the induced current. There are indications, too, that he may have realized the impulsive character of the deflections of the sine galvanometer, though he failed to analyze them properly, and was in error when estimating the magnitude of the electrical currents which he investigated.

CHAPTER V

STUDIES IN ELECTRICAL RESISTANCE

FOLLOWING the classical research which established the conditions upon which the magnitude of the induced electromotive force depends, Lenz then applied himself to equally exacting studies of electrical conductivity and resistance. These experiments were made in an interval between the first studies in electromagnetism, and later researches, which have more especially established his fame. So intimately were the studies in resistance related to those in magnetism, they require an adequate presentation in this work. Beside their intrinsic value, they enable the character of Lenz, as an investigator, to be estimated more to his advantage, than in his earlier work. Here are clearly shown his predominant characteristics: an ability to derive aptly an important generalization; the power of concentration required for exacting and exhaustive experiments; and skill in the methods of mathematical physics.

So extensive were these investigations, their adequate presentation and discussion would be a diversion from the purpose and limitation of this writing. At first, giving attention to the laws which apply to divided circuits, he solved a number of intricate problems for the resistance of compound circuits.*

However, it was in a paper read before the Academy of St. Petersburg, June 7, 1833, that he most clearly revealed his powers as an investigator. This paper was entitled, "On the Conductivity of Metals at Different Temperatures for Electricity."† In it he states: "I had anticipated that there would be found a relation between the expansion of the metal under the action of heat, and its lowered conductivity for electricity." It appears that, at some previous time, he had examined copper, brass, iron, and platinum to detect and measure the slight change. With no means then at his disposal could he find certain indications of such temperature change of resistance, as he had anticipated. The formulas which he employed for calculating the results of his experiments failed to yield a measurable temperature change. At first Lenz had concluded that these formulas were inadequate; upon further consideration he concluded, the internal resistance of the battery was a factor of such uncertainty, that it masked what the change in temperature effected in the resistance proper of the metal. When he had developed the experiments with induced currents, he found in the electromotive spirals the desired means for further investigations of the supposed change of resistance. He was now able to establish in the circuit under test, an electromotive force which was free from the factor of uncertain internal resistance which had perplexed him, when he had employed a voltaic battery for the earlier experiments. Being prepared, now, to ascertain the exact resistance of each element of the circuit, he at once established the existence of a temperature change in the resistance of metals, and found its magnitude with a considerable degree of accuracy.

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* Dove's *Repertorium,* Band viii.

† Poggendorff's *Annalen,* Band xxxiv. page 418.

The priority for the recognition and estimation of these phenomena properly belongs to Lenz, though he is not commonly appreciated as the original authority for this phase of electrical resistance.

As in his previous experiments, the results were formulated, and he derived from their data the expression,

$$Y_n = X + Yn + Zn^2;$$

which is the original suggestion for the now prevalent formula,

$$R_t = R_o (1 + \alpha t + \beta t^2).$$

Matthiessen is the preëminent authority who is usually quoted and instanced in connection with temperature changes in the resistance of metals. He properly is such an authority in matters regarding the specific resistance of certain metals; and, probably, by the association of these similar properties, he is usually referred to for the early value of the temperature coefficient of resistance.

Except by a few scholars the work of Lenz in this subject is either unknown or ignored; in the common omission of English text-books to cite authorities, it has come about that these researches of Lenz have been almost entirely neglected by them. As much interest should be shown in the original investigation of the temperature coefficient, as in the values of specific resistance.

The historical succession of authorities for the coefficient of the temperature change in resistance, is Lenz,* Becquerel,† Arndtsen,‡ Matthiessen,§ and Müller.‖

Lenz was in possession of a new and powerful means of investigation through his electromotive spirals; and he seems to have become eagerly absorbed in the whole question of electrical resistance. He had now a source of electromotive force whose resistance could be precisely and readily determined; and all the elements of the resistance of the circuit were open to complete investigation.

He had not proceeded far in these experiments without developing, for his immediate necessity, some unit quantity for the measurement of resistance; and it is remarkable that he improvised this in terms of a unit-length of copper wire with a prescribed cross-section.

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* Poggendorff's *Annalen*, Bände xxxiv. and xlv.

† Poggendorff's *Annalen*, Band lxx.

‡ Poggendorff's *Annalen*, Band civ.

§ Poggendorff's *Annalen*, Band cxv.; and, with C. Vogt, Band cxxii.

‖ Poggendorff's *Annalen*, Band ciii.

The paper describing these important experiments was read before the meeting of the Academy of November 28, 1834. It was entitled, "On the Laws of the Conducting Power of Wires of Different Lengths and Diameters for Electricity."* In this memoir Lenz discusses Ritchie's observations on "conductibility,"† and shows him to have been in error. He thereupon, undertakes a lengthy consideration of the similar work of Ohm ‡ and Fechner; and, in the course of these remarks, he employs simple algebraical methods to phrase the law of resistance for simple and compound circuits, using forms of analysis which are still accepted.

In the progress of this experimental research, Lenz determined resistances

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*Taylor's *Scientific Memoirs*, vol. i. pages 311–324.

† *Philosophical Transactions*, 1833, page 313.

‡ Schweigger's *Journal*, Band xlvi. 1826.

by the method of substitution. Had he attempted these measurements by using a battery for the source of electromotive force, the variation in the cell of internal resistance and potential would probably have deprived the results of coördination; and his investigations would not have supported those laws, which he was endeavoring to establish. But, again, his electromotive spirals furnished him with a reliable and unvarying source of electricity for ballistic galvanometry, and they gave to his experiments a definite and permanent value.

His algebraic analysis of the method of substitution begins with the familiar expression,

$$J = \frac{E}{W};$$

in which J was the symbol for the intensity of the current, and W was the resistance of a simple circuit, including the spiral, leading wires, galvanometer and a rheostat, which was set to its zero position. A length of the rheostat wire was then included in the circuit; and its resistance in terms of the unit copper wire was termed $\frac{l}{c}$. The intensity of the current was now J', and the relations of the quantities were,

$$J' = \frac{E}{W + \frac{l}{c}}.$$

The electromotive force generated by the spirals was constant throughout these tests. Then by evaluating E and equating its values, the resistance W, found with reference to $\frac{l}{c}$, was,

$$W = \frac{l}{c} \times \frac{J'}{J - J'}.$$

The circuit was now standardized with respect to this copper-unit of resistance. The wire, whose resistance R Lenz then wished to measure, was substituted for the wire of the rheostat by returning this instrument to its zero position.

Under these conditions, the intensity of the current was J'', which was then equated in the expression,

$$J'' = \frac{E}{W + R}.$$

By the process just employed, the resistance R was,

$$R = W \frac{J - J''}{J''};$$

and, by the substitution of the value of W from a preceding equation, finally R was found to be,

$$R = \frac{l}{c} \times \frac{J'}{J''} \times \frac{J - J''}{J - J'}.$$

For successive measurements of resistance it was only necessary to observe the current intensity J''. By proceeding in this manner he obtained values for the conductivity of silver, copper, gold, iron, tin, lead, platinum, and numerous other conductors. Despite the crude standards for current and resistance, these methods of Lenz were remarkably successful, and afforded results which closely approximated the constants that Matthiessen subsequently determined.

Though these investigations now appear to have been simple and the methods crude, yet, when regarded in connection with their historical setting, they are not without their significance. But in this particular paper, the author transcends the experimenter, and Lenz becomes convincing for his clear and concise grasp on the subjects involved; the scientist stands out before us, characterized by a fine and well-trained mentality.

Lenz refers, in this paper, to Ohm's law as an axiom; and, summarizing his own work, he states,

"The conductibility of wires of the same substance, is inversely as their length and directly as their cross-section."

Considering this relation as sufficiently established by his own experiments, he continues, in explanation,

"Still the methods for determining the conductibility of a substance by the induced electrodynamic current offers so great facility and accuracy of observation, that it appeared to me worth while to apply this method."

He had come to the conclusion, that the discrepancies in tests of conductivity, which had confused the results of other investigators, were caused by some variations in the resistance of the battery which they had employed. These various investigations, it must be recalled, were conducted by methods of substitution of resistance. In contrast with these results, Lenz considered his observations accordant and authoritative; and he attributed their reliability to the source of electromotive force which he had used. This he had kept uniform in intensity, and it had been free from variations in internal resistance.

At this time, he was not yet acquainted with those effects of an impulsive, or an alternating electromotive force, which are now phrased under the generic term, "impedance." Later on, indeed, he discovered these effects for himself and developed their characteristics with great clearness.

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Fortunately impedance effects did not interfere with these experiments. The circuits were low in reactance; yet, no doubt, certain small discrepancies, which he could not then explain, were to be attributed to this cause. With this exception, these methods of Lenz were remarkably simple and effective. One can not refrain from applying the word, brilliant, to the suggestion which led him to employ the electromotive spirals in these tests.

Again, this paper may be taken as a singularly clear illustration, that the first approximation of a scientific principle may be very simply obtained; and this crude approximation then carries with it a degree of fame — not to say a peculiar reverence — which is withheld from its subsequent and elaborate verification.

The student, who is merely searching for facts, concise statement of principles, or convincing experimental verification, may have little desire to delay over these simple beginnings of ideas, which are now so clearly grasped and commonplace. A higher order of intelligence, however, will follow with enthusiastic interest these gropings of the earlier scientists after truths, whose gravity and importance they could only vaguely apprehend, as they proceeded from one discovery to another suggested by it.

CHAPTER VI

LENZ'S LAW

THOUGH Lenz was not destined to receive wide and general recognition as one of the original discoverers of the phenomena of electricity induced from magnetism, his chief fame was to rest upon one terse and valuable generalization. This principle, which perpetuates his name, is the Lenz's Law of all the text-books on physics and electricity. It was, for its day, a supreme philosophical contribution. New facts were fast accumulating, which required adequate generalization to avoid confusion. An effort had, indeed, been made to assign the new facts to a definite principle, but the attempt was not convincing.

It remained for Lenz to pronounce the adequate formula, which should serve to connect the new phenomena of electricity induced from magnetism into a philosophical system ; and which should later place these same phenomena under the broad generalization of the Conservation of Energy, soon to be developed and received. It was to be the good fortune of Lenz to pronounce a law, which should eventually prove eminently practical in the application of the basal principles of electromagnetic induction to electrical machinery.

This law was first announced in the paper, "On the Direction of Galvanic Currents Which Are Excited through Electrodynamic Induction."* It was read on November 29, 1833, before the Academy at St. Petersburg.

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*Poggendorff's *Annalen*, 1834, Band xxxi. page 483. The exact title of this classical paper reads, "Ueber die Bestimmung der Richtung der durch elektrodynamische Vertheilung erregten galvanischen Ströme."

Lenz was already familiar with those generalizations which Faraday tentatively derived.* Certain exceptions were taken in the introduction to this paper, to the rules for determining the direction of the current, as given by Faraday. These rules Lenz considered inadequate, if not actually erroneous. His own reading of similar rules was:

I. "Between parallel conductors, one of which carries a current, an opposing current is induced upon approach toward, and a current similarly directed upon withdrawal from the first conductor."

II. "When a conductor is moved near a magnet, the direction of the induced current depends upon the manner of cutting the magnetic curves."

To these statements Lenz objected at length, and insisted that Faraday had here stated two wholly different laws to define one and the same phenomenon. To fortify his criticism Lenz appealed to what he called, "Ampère's beautiful theory," which regarded a magnet as a system of circularly directed currents.

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* He had read these statements from Poggendorff's *Annalen*, No. 5, 1832. See *Experimental Researches*, volume I. pages 32 and 33.

There is something sinister in the lavish praise he here applies to Ampère's theory. It is not usual to find a scientist praising the work of a contemporary with enthusiasm. Lenz proved no exception in this instance; his praise of another was something of a pretence to lend emphasis to a personal attack which he was prosecuting.

If the objection is carefully considered, which he brought against the rules of Faraday, it must be confessed that the stricture appears trivial. Its impression leaves Lenz open to the implication, that he wholly failed to grasp the important significance of the magnetic curves, or lines of force, which Faraday had supposed to constitute the magnetic field. It also offers a suspicion that

Lenz was intent on some opportunity to criticise the magnificent discoveries and the significant work of Faraday. It is, indeed, singular to find Lenz assuming this attitude of opposition toward what should have been evident from his own experiments. His first investigations should have proved conclusively to him, that the cause of induction lay wholly apart from, and without, the circuit in which it took place. But it appears that the conception of a medium, through which the induction had occurred, had not intruded itself upon his attention.

In this discussion, which Lenz had read, Faraday had clearly considered a magnet and a wire carrying a current identical, or at least equivalent so far as they were both accompanied by magnetic curves. His statements referring to two variations of conditions were, then, very properly phrased into one general law, of which the conditions presented were corollaries.

But Lenz, missing the conception of a medium and confining his thoughts to the mere attraction and repulsion of a magnet toward a conductor carrying a current, considered a magnet and such a conductor to be identical, according to Ampère's hypothesis. He was correct in thus assuming that both the relations, which he had quoted, were aspects of one and the same phenomenon.

The philosophical error into which Lenz fell originated from a too literal interpretation of Ampère's hypothesis. This prevented him from appreciating and accepting Faraday's conception of a magnetic field: though nearly all contemporary scientists and mathematicians were at first equally averse toward the assumption of lines of force.

Since the processes by which Lenz finally arrived at his classical law, are largely dominated by a criticism of Faraday's work, the objections themselves become significant. These will presently be given in some detail following their original order.

In attacking the generality of Faraday's tentative rules for the direction of the current, Lenz instances the case of a conductor placed perpendicularly to a second one in which a current circulates. According to the interpretation which he placed on Faraday's rules, no current would be induced in this conductor, when moved parallel with itself and, in its plane of movement, parallel with the inducing conductor.

Perhaps this stricture was obvious for one influenced by the particular view of such phenomena entertained by Lenz; but it clearly proves him to have been empirical, rather than philosophical, in his thinking: and what should have led his thoughts to the conception of a field of force, in the end only suggested to him an empirical generalization.

In order to illustrate the sense of direction of the induced current with respect to the polarity of the magnetic field establishing it, Faraday had resorted to the illustration of a silver knife-blade. He conceived this to be moved in such manner that the notched side lay upward as it was moved along a bar, or a cylindrical magnet, and its edge cut the lines of force. Lenz did not spare something of a sneer at the homely illustration. He turned it against its originator, as an instance of the stricture he had placed on the supposedly general law.

Since the knife-blade, in fact, was placed perpendicularly to the axis of the inducing magnet, he asserted that there should have been no current induced in it. This objection was valid, only, for the particular case of movement near the middle of a very long straight magnet.

The illustration, as used by its author, however, was an apt one; and had in it something of the inspiration of genius on the part of Faraday. Its criticism showed the absence of this quality on the part of Lenz.

However, the further study of Faraday's papers, and Nobili's comments on them, added a new significance to the experiments which Lenz himself had performed. From such sources the impression grew upon him, that all related phenomena of electrodynamic currents might be simplified and brought together under a statement of a single terse and comprehensive law.

Inspired and directed by this idea, with characteristic energy Lenz entered upon the repetition, verification, and extension of the experiments of Faraday. In particular, these were the experiments that had immediately succeeded those investigations, which had assured his discovery of the induced current.

Through the investigations which he now prosecuted, Lenz was finally led to the elegant — though for him empirical — generalization, which has made his name familiar to students of electricity, and which has especially given a classical value to his work. Stated in the words of its author, Lenz's Law reads,*

"Wenn sich ein metallischer Leiter in der Nähe eines galvanischen Stromes oder eines Magneten bewegt, so wird in ihm ein galvanischer Strom erregt, der eine solche Richtung hat, dass er in dem ruhenden Drahte eine Bewegung hervorgebracht hätte, die der hier dem Drahte gegebenen gerade entgegengesetzt wäre; vorausgesetzt, dass der ruhende Draht beweglich nur in Richtung der Bewegung und entgegengesetzt wäre."

Perhaps this law is more familiar to the general student in the awkward phrasing, "If the relative position of two conductors *A* and *B* be changed, of which *A* is traversed by a current, a current is induced in *B* in such a direction that, by its electrodynamic action on the current in *A*, it would have imparted to the conductors a motion of the contrary kind to that by which the inducing action was produced."*

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* Poggendorff's *Annalen*, Band xxxi. page 483.

More tersely expressed the law states, *The electrodynamic action of an induced current opposes equally the mechanical action inducing it.* When expressed in this form, it is readily recognized to be a corollary of the general law of the Conservation of Energy. Had Lenz anticipated, even remotely, the significance of the equivalence in this mechanical action, his fame as a philosophical physicist would have been established. But the equivalence in the mechanical relations between the inducing and the induced currents seemingly did not occur to him ; his attention was absorbed

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*Ganot's *Éléments de Physique*, translated by E. Atkinson, eleventh edition, page 828.

by the mere mechanical relation that predominated this particular group of phenomena.

Some fourteen years later, when the principles of the Conservation of Energy were beginning to be grasped, Helmholtz, indeed, showed that this law was a necessary corollary of the more general principle.

A certain similarity between Faraday and Lenz has a passing significance at this juncture of the narrative. Maxwell gave to the work of Faraday its final philosophical and mathematical significance; and, in like manner, Neumann extended the mathematical expression and formulation of the work of Lenz. By reverting to Neumann's writings the attitude and spirit of certain scientists, who were contemporaries of Faraday and Lenz, is obtained; and their conceptions of induced currents and the phenomena of electromagnetic induction are placed in contrast with the prevalent concise ideas of the science of applied electricity. The following passage is translated from a discussion of Lenz's law, which Neumann published in 1845, in the introduction to a work on the theory of induced currents.*

"From the law of Lenz: the action which the inducing current, or magnet, exercises upon the induced conductor, if the induction results from the movement of the latter, is always in the nature of a check on its motion. And, further, the strength of the momentary induction is proportional to the velocity of the movement. From these considerations, the general law of linear induction may be derived. Its numerical expression is,

$$E . Ds = -\epsilon v C . Ds.$$

"In this formula, *Ds* is an element of the induced circuit, and *E . Ds* is the electromotive force induced in the element *Ds*; v is the velocity with which *Ds* is moved, while, according to the direction in which *Ds* is moved, *C* denotes the magnitude of the resolved action of the induction upon *Ds* when unit current is considered to be flowing in this element. The magnitude of ϵ is independent of the activity of the induced conductor, and in the case of linear induction may be regarded as a constant; but it is a function of the time, which decreases rapidly if its argument attains a finite value."

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* *Die mathematischen Gesetze der inducirten elektrischen Ströme*, by Franz Neumann (1845), page 4; Leipzig, 1889.

In these statements Neumann expresses the views of a merely analytical mathematician; he is wholly occupied with mere relations, and is not concerned to search for a physical explanation of the phenomena discussed, or to look for their causation. These conceptions are based on certain experimental results; and his endeavor was confined to establishing relations between various data.

But no conviction is carried by so narrow views; what is lacking in them is that physical aspect so finely expressed by Faraday in his conception of magnetic curves, or lines of force.

In this respect the preëminence of Faraday over Lenz, especially, and philosophers of the type of Neumann, is emphasized, when it is considered that Lenz was so fully occupied with Ampère's theory of the magnet, that he did not go beyond it. However Faraday, as fully conversant with "M. Ampère's beautiful theory," as he calls it, found this inadequate and established a more rational conception of the magnet and its field of influence.*

A further phrasing of his Law was given by Lenz in this paper. While it is more philosophical and comprehensive it yet misses the perception of its greatest significance; it is still a law merely defining the direction of the effect of movement, though it vaguely approaches the relations of energy in the circuits.

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* *Experimental Researches*, § 38, page 10.

This second statement reads: "To each phenomenon of movement by electromagnetism, there must correspond an electrodynamic distribution. Consequently it is only necessary to produce motion through other means in order to induce a current in the movable conductor, which shall be opposed in direction to that so produced in the induced conductor of the electromagnetic tests."

When the experimental evidence is examined for the establishment and the proof of the law, one is impressed with the painstaking thoroughness of Lenz's work. While availing himself of data already developed, he verified and extended every aspect of the subject. His method of attack in these investigations was both logically and comprehensively planned, and his results left nothing to be added by others. For the most part, he proceeded by comparison with the experiments of Faraday, Ampère, and Nobili.

His comparative statements, which follow, in the first instance give the electrodynamic effect; and, in the second, the reverse dynamic effect of induction:

A. According to Ampère's rule for parallel currents, two similarly directed currents mutually attract; while they repel each other, if oppositely directed.

a. From Faraday's experiment: if a current is flowing in one of two parallel conductors, when the second conductor is caused to approach it, a current of opposed direction is induced in this circuit; and conversely.

B. By what subsequently became the principle of the electrodynamometer: if two circular currents are vertically placed with their planes at right angles, the conductors having a common diametral axis of rotation, and the one is fixed while the other conductor is free to move, — the movable circuit will rotate until the two planes of the conductors coincide and the currents have a common direction.

b. If two circular conductors have the axial arrangement of the preceding case: when the movable coil is rapidly turned from a position of quadrature to one of coincidence with the fixed coil, a current is induced in the movable coil, which is opposed in direction to that flowing in the inductor.

This relation was anticipated by Lenz and experimentally established by him. In it will be recognized the principle of the earth inductor. In the apparatus employed by Lenz the circular frames were each wound with twenty turns of copper wire.

C. From an experiment of Nobili:* if a fixed rectilinear conductor sufficiently long to be free from end effects, has a short conductor arranged at right angles with it and free to move parallel with

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*Poggendorff's *Annalen*, No. 3, 1833, page 407.

itself; and if this conductor is connected as a shunt from the fixed conductor, so that the direction of its current is from this circuit, — the shunt will move in the direction of the current in the fixed conductor; and conversely.

c. When a conductor is placed in the relative position of the shunt in this arrangement, and is moved in the direction of the flow of current in the inducing fixed conductor, an induced current results, whose direction of flow is toward the inductor, or opposed to the relative direction in *C;* and conversely.

Lenz states, that he considered these were the principal cases in which a galvanic current exercises an electrodynamic and an electro-inductive action upon an appropriate conductor.

He next proceeded to examine the important cases of mutual reaction that exist between currents and magnets. In his account he prefaces the recital with a discussion of Ampère's rule for finding the direction of an induced current by the fiction of a man swimming in the circuit; and, with something like perversity, he proposed a modification of the rule, derived from his own Law. His statement of this rule was,

"If a conducting circuit is moved before the north pole of a magnet, an induced current results from the electrodynamic distribution, which streams through in a direction from the head to the feet, if one is imagined to be in the circuit and turned so as to view the north pole, provided that the motion of the conductor is toward the right hand."

Just what conception Lenz entertained of this electrodynamic distribution (elektrodynamische Vertheilung) can not be obtained with certainty from his paper; if, indeed, he went so far as to derive for himself a working conception of it. In his experiments he seems to have employed the vague idea of electrodynamic distribution in much the same sense as Faraday did his precise and practical conception of magnetic curves.

Proceeding to discuss the reactions between magnets and currents, Lenz resumes his topical argument:

D. According to Oersted's discovery, when a conductor charged with a galvanic current is placed horizontally and axially above a magnetic needle, the direction of the current being from the south pole toward the north, this latter end of the needle will be deflected toward the right hand.

d. If a linear conductor is horizontally and axially placed above a magnetic needle at rest, and the needle is suddenly deflected by some means, a current is induced in the conductor, whose relative direction will be opposed to that stated in the conditions in *D;* or, if the north pole of the needle is deflected toward the right, the flow of the induced current will be from the north toward the south pole; and conversely.

To prove this experimentally, Lenz employed a frame of wood, one foot square; this was wound with a large number of turns of fine, silk-covered copper wire.

E. As shown by the experiment of De la Rive, if a circular conductor carrying a current is arranged to move freely, and surrounds a long cylindrical magnet, and the direction of its current coincides with the resultant direction of the molecular currents of Ampère's theory, — the conductor will move toward the center of the magnet. If the direction of the two currents is opposed, the conductor will then tend to move from the center and over the nearer pole.

e. If a circularly wound conductor is thrust over the end or pole of a long bar magnet, a current will be induced in it, whose direction is opposed to that of the Amperian currents of the magnetic pole. In case the circular conductor is removed quickly from a position in which the coil surrounds the magnet, the induced current will then follow the direction of the Amperian molecular currents.

This result Lenz did not verify experimentally, but he was satisfied to assume it from Faraday's statement.

F. By the experiment of Barlow with a rotating wheel: if a bar magnet is held diametrically over the wheel, with its north pole toward the left hand, and a current flows from this point on the periphery of the wheel toward the center, the wheel will rotate in a clockwise direction; and it will rotate in the opposite direction by changing the relations between the flow of the current and the magnetic pole.

f. Varying this experiment, while he maintained the axis of the wheel in a vertical position, Faraday placed the north pole of a curved magnet over a point on the periphery of the wheel and the south pole beneath it. Upon turning the wheel in a clockwise sense, a current was induced in it, whose flow was from the center toward the periphery of the wheel; and conversely.

G. A cylindrical magnet which is arranged to rotate about its axis, according to Ampère's experiment, will turn in a clockwise sense, regarding its north pole, when a current is made to pass from the north pole toward the center of the magnet.

g. Faraday demonstrated, when such a magnet is rotated in a clockwise sense, and a galvanometer is connected with contacts at the center and the upwardly pointing north pole of the magnet, a current is induced in the magnet, whose direction of flow is from the center and toward its north pole; and conversely.

The logic of this topical argument amounts to a convincing demonstration of the Law of Lenz. With this contribution toward the exact classification of the essential relations of electrodynamic action, the classical work of Lenz, as one of the founders of this science, is brought to a close. The foundations of the new science were now firmly laid; and the subsequent work of Lenz was devoted for the most part to the application and extension of these important scientific phenomena.

CHAPTER VII

SUBSEQUENT EXPERIMENTS AND CONTRIBUTIONS

FROM the fact that the principles of electromagnetism have universally received practical applications of such bewildering variety and profound importance, the history of the science of these principles is necessarily marked by sharp divisions. The initial period of discovery was one of the establishment of principles and theories, rather than application of them.

The application of the new principles, which soon followed upon their discovery and knowledge, resulted in numerous and varied lines of exact science. The applications themselves supplied the zeal and interest for new and more searching investigations of the older facts; and in doing this they reacted upon the older theories, eliminating or confirming the former conceptions, until a rational and scientific theory was obtained.

To the initial period of this science belong the labors of Faraday, Henry, and Lenz. But of these scientists, it was more especially Lenz who developed his investigations into the new and broadening lines of applied electromagnetism. On the part of Henry, this practical interest led him to the discovery of the essentials of the electrical telegraph.

A critical account of the scientific work of Lenz would consequently deal with almost the entire history of electromagnetism for the thirty years succeeding 1833. So untiring was the zeal of this investigator, and his intelligent interest was so broad and varied, that there were few essential phases of the theory and applications of electromagnetism, which he did not investigate with more or less thoroughness. In this respect he was the prototype of our own scientists, who have been so illustriously connected with the advancement of applied electricity.

The scope of the present work, however, is limited to the initial period of this history of electromagnetism. An endeavor has been made to show the relation and influence of the labors of Lenz upon the discoveries in, and the establishment of, the science of electromagnetism. It is naturally concluded, then, when the subject has been carried to that period in the development of the science, in which new direction and impetus were given through the practical application of its principles.

To leave the work of Lenz at this stage, and not indicate, at least briefly, the character and value of his subsequent labors, would be an injustice to the real estimation of this eminent scientist. To preserve the intention of this volume, and, at the same time, afford the reader a means of becoming acquainted with the entire scientific work of Lenz: a brief summary of his later and more important investigations is given; and this is supplemented by extensive references to his bibliography.

As an investigator Lenz is reasonably entitled to the honor of being considered one of the foremost scientists who established the principles of electromagnetics. The sensationalism of discovery appears not to have been reserved for him; so that he has but slowly come into his own, and been awarded adequate appreciation. Throughout his experiments and writings there is frequent evidence that he lacked originality and philosophical intuition; and that his work was largely shaped and directed by the hints which he obtained from other scientists, rather than it sprang from his personal initiative. Neither in his contributions nor investigations can he be called brilliant; he, perhaps, did little, if anything, which would not subsequently have been done: yet all his work was of that solid and enduring character, which forms the foundation of all science. His methods were painstaking and exhaustive: he verified, extended and formulated; yet there also appeared several errors and mistaken conclusions in his work.

His contemporary influence was wide and pronounced. His papers exercised a general influence upon the development of electromagnetics, especially in France and Germany; and, at least in one respect, they were the most notable of the period. In this essential respect he was governed by the quantitative method of investigation. As a result, there is much in the writings of Lenz which is, even now, modern in spirit; especially when compared with the contributions of Faraday, Henry, and their contemporaries.

In addition to the contributions to electro-physics, other than magnetism, which already have been described, Lenz investigated the Grove battery quantitatively.[*] He also applied himself to certain aspects of electrolytic polarization and the internal resistance of batteries.[†] Following the discovery of thermo-electricity by Seebeck;[‡] and the observation which Peltier[§] made of the change in temperature at the junction of heterogeneous conductors: Lenz subsequently made a notable contribution on the cooling effect of a current passed through the junction of a thermo-electric couple.[‖]

Soon after Joule announced his law for the heating effect of an electrical current, Lenz verified and extended the investigations in a precise and remarkable manner.[¶] His apparatus was simple;

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[*] Poggendorff's *Annalen*, Band lxvii.

[†] Poggendorff's *Annalen*, Band xc.

[‡] Poggendorff's *Annalen*, Band vi.

[§] *Annales de Chimie et de Physique*, tome xxxix. page 1212.

[‖] Poggendorff's *Annalen*, Band xliv.

[¶] Poggendorff's *Annalen*, Band lxi.

it was a small flask, exposed to the air and inverted over a heating spiral of platinum wire. The temperatures were observed with a sensitive and accurately calibrated thermometer. The flask was partially filled with an alcoholic solution of eighty-five per cent. strength.

To eliminate the cooling influence of the air, the apparatus was cooled to a temperature as far below that of the surrounding air, as the intended elevation of the temperature was above this. Not satisfied with one result, Lenz substituted wires of other metals for the platinum, and detected slight variations in the comparative results. He finally reconciled these by taking into account the specific heat of these wires themselves. Deducting this loss of heat he found, that the heat developed in an electrical circuit was proportional to the resistance and the second power of the intensity of the current. Though this testing of different metals in succession, may appear an insignificant detail; yet it shows the painstaking care of this investigator.

Lenz in the beginning was fortunate to have recognized in his electromotive spirals an instrument for investigation, which was capable of a wide and exact application. By the use of these spirals he conducted numerous measurements and tests, which very closely resembled modern permeametry in the simpler forms of its methods and apparatus.

With the aid of Jacobi,* he found a parabolic relation between the strength of magnetization of an iron rod and the intensity of the magnetizing current employed. These investigations included the determination of the magnetic moment for various types of magnets; and the influence of the size of the section on the magnetic moment.

Among his experiments in applied electromagnetism, there was a series † of investigations to which Lenz applied himself with the greatest enthusiasm, and, certainly, with signal success: this was a study of the influence of the speed and the magnetization of certain magneto-electric generators on their maximum output and electromotive force.

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* Poggendorff's *Annalen*, Bände xlvii. and lxi.

† Consult the bibliography of the following chapter.

He was able to establish experimentally: that the plane of commutation does not coincide with the geometrical north-south plane of the commutator, when the generator operates with a considerable load. Searching for the cause of this deviation he found, that the degree of displacement was, in a sense, a function of the intensity of the current. He was thus the first investigator to discover the existence of armature reaction, and explain it clearly.

Another interesting fact developed in this series of papers, is the presentation of the elementary characteristics of the magneto-electric generator. Again in this graphical method, Lenz displayed the true attitude of the discoverer, and introduced, what is now so important a feature of testing dynamos and motors.

Following suggestions from the experiments and writings of Weber, Lenz thoroughly worked out the conditions for a maximum output of current from a magneto-electric generator. Perhaps the most important of these later experiments were those in which he studied the characteristics of the armature, and the field of these machines. As a result he was enabled to chart the sinusoidal electromotive wave-form; the position and magnitude of a wave-form due to counter-electromotive force; and the curve of magnetization in its initial position, and when shifted by the armature reaction.*

In these papers the data are charted in the customary method now so largely used. So far as the first practical charting of these properties of a generator are concerned, probably Lenz was the first scientist to employ those graphical methods, which have proven so valuable, and are now so universally employed for studying the properties and performance of an alternating-current generator.

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* Poggendorff's *Annalen*, Bände xlvii. and lxi.

Though the purely scientific and the practical investigations of Lenz in electricity and magnetism, have not met with general appreciation, and they are not so fully recognized in English text-books, as they deserve; he has been accorded frank recognition in German scientific literature. Wüllner* in particular among these writers, has shown a generous recognition of the work of Lenz, and usually describes it at some length.

On comparing the significance of this later work of Lenz with his earlier investigations, there is evidence that he was a better scientist than a discoverer. In themselves these papers on practical electromagnetics are exceptionally interesting and instructive; while their study discloses some of the most important details in the history of the development of the exact knowledge of the dynamo.

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* These references are comprised in the fourth volume of the *Lehrbuch der Experimentalphysik*, by Dr. Adolph Wüllner, fourth edition; Leipzig, 1886.

CHAPTER VIII

BIBLIOGRAPHY

A DETAILED bibliography of the scientific contributions of Lenz is presented in this chapter.* The citations from the foot-notes in the text, are repeated for convenience in consulting the various references, and in the use of alternative publications.

Though many of the papers which receive mention in this list, have no direct relation with the subject-matter of the text, they are inserted in their chronological order to complete the record of the bibliography. This method has been preferably adopted so that the entire scientific work of Lenz might be presented for the benefit of those, who may desire to study this more particularly; and in the anticipation that it may lead to an adequate estimation of the extent and value of the scientific labors of the author of these contributions.

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* *Catalogue of Scientific Papers*, (1800–1863), compiled and published by the Royal Society of London.

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10. Bemerkungen gegen den in diesen *Annalen* enthaltenen, wider mich gerichteten Aufsatz des Hrn. MUNCKE über Thermoelektricität des Glases. Poggend. Annal. xxxv., 1835, pp. 72–80.

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41. Ueber ein neues Anemometer. St. Pétersbourg Acad. Sci. Bull. VI., 1863, col. 184–91.

In addition to this remarkable series of individual contributions, memoirs and reports, Lenz collaborated in publication with other scientists. The number of these papers is not large; and, for the most part, they were read in the years 1839–48, when Lenz had already become a recognized investigator.

Among these writings there are two contributions on the laws and properties of the electromagnet, in which Jacobi was associated with Lenz; and one on galvanic polarization, with Saweljev as a collaborator.

CHAPTER IX

A BIOGRAPHICAL NOTE

IT is a practical consequence of the nature of the labors of the scientist, that his work should be regarded as impersonal. It is custom, rather than necessity, which enforces this condition; and, perhaps, mistaken zeal and a lack of finer appreciation on the part of scientists as a class, is responsible for the peculiar estimation of the scientist and his work.

Within recent years the avenues of publication for scientific literature have multiplied to such an extent, that the passing of a scientist elicits from his friends a kindly record of his life and achievements; and, frequently, generous appreciation of his work. A fund of historical material is thus being accumulated, which will come easily to the hand of the future historian.

Yet no thoughtful reader can fail to be impressed with the reflection that these notices are too brief. Generous as they often are, they do not adequately portray the man who has dominated the work.

Compared with the attention accorded to men of equal eminence in pursuits which are more attractive to the general public, the life of the scientist elicits slight attention. Of all classes of men the scientist is more severely required to be known only through his work, and for his work's sake; while the personality, which dominated his labors and gave them both color and value, is but vaguely seen through the meagre details of biographical notices.

Such observations are pertinently suggested by the attempt to study the life of Lenz. With the present resources of scientific literature, if the means are inadequate to portray the man, the resources of the past can yield little but disappointment. The past facilities for publication were few and limited rather to bald description of investigation and work; while the biographical memoir of the scientist was almost unknown.

An account of the work of Lenz recently written by his own countryman, bluntly begins the record with the year, 1833. It is silent about the interesting years of education and development; and proceeds to recount the minutia of experiment and observation, much as though the subject of the sketch were some impersonal worker.

The most careful research has failed to restore to our notice an extended biographical account of Lenz, either as scientist or man. The material which is available for the study of his life, is the chance brief biographical notice of the handbook and the encyclopædia; and in one instance, at least, this is absurdly erroneous.* To these notices are to be added such inferences of the life and character of the man as are revealed in and through his peculiar methods of work, and his own writings. Throughout this book frequent observations have been made upon suggestions of this nature: and it remains to supplement these by such a brief biographical account, as is possible.

Heinrich Friedrich Emil Lenz was born on February 12, 1804, at Dorpat or the Russian Yurief. However interesting it would be to know something of his parentage and early environments, no knowledge of these factors in his life is at present available.

Apparently his early educational advantages are equally unknown, at least

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*Lippincott's *Biographical Dictionary*, edition of 1895. See the author's comments on this notice, in the *Journal of the Franklin Institute*, April–May, 1903.

to all but a few, it may be, of his own countrymen. According to one account he evidently studied theology in the beginning of his professional education; but, abandoning this calling, he devoted his attention to the natural sciences, and more especially to physics. This bare detail of his life suggests an interesting chapter in his career, which is sealed to inquiry.

Upon the completion of his academic course Lenz received the degree of doctor of philosophy. His successful and diligent devotion to science is inferred from the event, that, when only twenty years of age, he accompanied O. von Kotzebue, the son of the celebrated poet, on the second of his voyages of investigation around the world. On this expedition Lenz traveled in the capacity of a naturalist; and, upon his return, he presented a valuable report of his observations to the Imperial Academy of Sciences at St. Petersburg.

It was probably in recognition of the value of such scientific work, that Lenz was elected an associate member of the Academy in 1828. The Academy bestowed on him in 1834 the further honor of election as a member, or fellow; and evidently this promotion was in recognition of his initial experiments and his papers on electromagnetism.

About this time Lenz became professor of physics at the University of St. Petersburg. In addition to this permanent office, he was also the sometime professor of physics at the Pedagogical Institute, and an artillery school of that city. While that he was broader than the mere specialist, is shown by public offices which he held: at one time he was the rector of the University, and also served as its counsellor.

Though a voluminous writer, Lenz published little beyond reports, papers, and memoirs descriptive of his travels, observations, experiments, and various investigations. In 1864, however, he published a *Handbook of Physics*, which was issued in two volumes.

About the time of the publication of this textbook his health became seriously impaired. Hoping that travel would benefit him, and to escape the rigor of the northern winter, Lenz undertook a journey to Italy; but he failed to regain his strength; and, finally, on February 10, 1865, he died at Rome.

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